removed, and it is advisable to make the tensions each equal to half the weight of one of the pulleys with its adjustable frame.) The machine is now ready for use. To use it, pull the cords simultaneously or successively till lengths equal to $e_1, e_2, \ldots e_n$ are passed through the rings $E_1, E_2, \ldots E_n$, respectively.

The *pulls* required to do this may be positive or negative; in practice, they will be infinitesimal, downward or upward pressures applied by hand to the stretching weights which (§) remain permanently hanging on the cords.

Observe the angles through which the bodies $B_1, B_2, \ldots B_n$ are turned by this given movement of the cords. These angles are the required values of the unknown $x_1, x_2, \ldots x_n$, satisfying the simultaneous equations (I).

The actual construction of a practically useful machine for calculating as many as eight or ten or more of unknowns from the same number of linear equations does not promise to be either difficult or over-elaborate. A fair approximation being found by a first application of the machine, a very moderate amount of straightforward arithmetical work (aided very advantageously by Crelle's multiplication tables) suffices to calculate the residual errors, and allow the machines (with the setting of the pulleys unchanged) to be re-applied to calculate the corrections (which may be treated decimally, for convenience): thus, 100 times the amount of the correction on each of the original unknowns, to be made the new unknowns, if the magnitudes thus falling to be dealt with are convenient for the machine. There is, of course, no limit to the accuracy thus obtainable by successive approximations. The exceeding easiness of each application of the machine promises well for its real usefulness, whether for cases in which a single application suffices, or for others in which the requisite accuracy is reached after two, three, or more of successive approximations.

December 12, 1878.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

Dr. Philipp Hermann Sprengel was admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

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I. "On the Flow of Water in Uniform *Régime* in Rivers and other Open Channels." By JAMES THOMSON, LL.D., D.Sc., F.R.S., and F.R.S.E., Professor of Civil Engineering and Mechanics in the University of Glasgow. Received August 15, 1878.

In respect to the mode of flow of water in rivers, a supposition which has been very perplexing in attempts to form a rational theory for its explanation, has during many years past, during at least a great part of the present century, been put forward as a result from experimental observations on the flow of water in various rivers, and in artificially constructed channels. It was, I presume, put forward in the earlier times only as a vague and doubtful supposition; but, in later times it has, in virtue of more numerous and more elaborately conducted experimental observations, advanced to the rank of a confirmed supposition, or even of an experimentally established fact. This experimentally derived and gradually growing supposition was perplexing, because it was in conflict with a very generally adopted theory of the flow of water in rivers which appeared to be well founded and well reasoned out.

That commonly received theory, which for brevity we may call the *laminar theory*, was one in which the frictional resistance applied by the bottom or bed of the river against the forward motion of the water was recognized as the main or the only important drag hindering the water, in its downhill course under the influence of gravity, from advancing with a continually increasing velocity; and in which it was assumed that if the entire current is imagined as divided into numerous layers approximately horizontal across the stream, or else trough-shaped so as to have a general conformity with the bed of the river, each of these layers should be imagined as flowing forward quicker than the one next below it, with such a differential motion as would generate through fluid friction or viscosity, or perhaps jointly with that, also through some slight commingling of the waters of contiguous layers, the tangential drag which would just suffice to prevent further acceleration of any layer relatively to the one next below it. Under this prevailing view it came to be supposed that for points at various depths along any vertical line imagined as extending from the surface of a river to the bottom, the velocity of the water passing that line would diminish for every portion of the descent from the surface to the bottom.

The experimentally derived and perplexing supposition for which no tenable theory appears to have been proposed, though the want of such a theory has been extensively felt as leaving the science of the flow of water in rivers in a state of general bewilderment, is, that inconsistently with the imagination of the water's motion conceived

under the laminar theory, *the forward velocity of the water in rivers is, in actual fact, sometimes or usually not greatest at the surface with gradual abatement from the surface to the bottom;* but that when the different forward velocities are compared which are met with at successive points along a vertical line traversing the water from the surface to the bottom, it may often be found that the velocity increases with descent from the surface downwards through some part of the whole depth, until a place of maximum velocity is reached, beyond which the velocity diminishes with further descent towards the resisting bottom.

That the superficial stratum of water flowing downhill under the influence of the earth's attraction should not have its forward velocity continually accelerated until, by its moving quicker than the bed of water on which it lies, a frictional drag would be communicated to it from below, by that supporting bed of water, sufficient to hold it back against further acceleration, has appeared very paradoxical. In various cases, during a long period of time, the alleged result appeared so incredible that the experimental evidence was doubted, or was dismissed as untrustworthy. In some cases the phenomenon was admitted as a fact, but was attributed to a frictional drag or resistance applied to the surface of the water by the superincumbent air, even in case of the air being at rest with the water flowing below, or more strongly so when the wind might be blowing contrary to the motion of the river.

Omitting to touch on the experimental results, and the opinions of various investigators in the older times, as I have not had sufficient opportunity to scrutinise them in detail, I have to refer to the investigations conducted at about the year 1850 by Ellet on the Mississippi and Ohio Rivers.* He was led to the conclusion† from his own experiments on the Mississippi, that the mean velocity of that river (or at least the mean velocity of the great body of its current, as the part near the bottom or bed of the river had not been definitely included in his researches) instead of being less, is in fact greater than the mean surface velocity. He attributed this phenomenon, which he regarded as indubitably proved, and which if true must certainly be very remarkable, to a frictional drag or resistance, against the forward motion, applied to the surface of the water by the atmosphere in contact with the surface. Like suppositions had previously been made by some observers and theoretical investigators in Europe, as may be gathered from D'Aubuisson "Traité d'Hydraulique," 2nd edition, 1840, p. 176, and from other sources of information.

* Ellet on the "Mississippi and Ohio Rivers." Philadelphia: 1853. This is a republished edition of a Report to the American War Department by Ellet on his investigations, which were made under authority of an Act of Congress.

† Pages 37 and 38 of the book referred to in the preceding note.

Other experimental researches on the flow of the Mississippi River, much more elaborate than those of Ellet, were made in the period between 1850 and 1861 by Captain Humphreys and Lieutenant Abbot, with others acting under authority from the American Government, and an account of them was published as a Report by Humphreys and Abbot in 1861.* These experiments and the investigations exhibited in the report, where the observed results are combined in various ways so as to bring out average results and more or less probable conclusions for various circumstances, lead very clearly and very convincingly to the conclusion that ordinarily the maximum velocity is not at the surface but at some depth below it, usually much nearer to the surface than to the bottom, and often at some such depth from the surface as $\frac{1}{4}$ or $\frac{1}{3}$ of the whole depth of the water. These investigators (Humphreys and Abbot) show further (at pages 285, 288, and 289 of their Report) that this phenomenon is not wholly nor even mainly due to any frictional resistance applied by the superincumbent atmosphere to the forward flow of the surface of the water; because they found that even when the wind is blowing in the direction of the river current, and advancing at the same velocity as that current, so that the air lies on the surface of the water without relative motion, the phenomenon manifests itself almost in as great a degree as when the air is lying at rest relatively to the land; and found yet further that the phenomenon still manifests itself even when the wind is blowing in the direction of the flow of the river much faster than the current, so that it blows the water surface forward instead of applying a resisting drag or backward force to the surface.

At about the middle of the present century very important experiments on flowing water were made in France by Boileau, and by Darcy and Bazin; and elaborate accounts of these researches were published.†

The experiments comprised among the researches of Boileau and of Darcy and Bazin, to which I have to refer as bearing on the special subject of the present paper, relate to the flow of water in long channels and conduits constructed artificially, some in wood and some in masonry and other materials. The channels or conduits in different cases were of widths comprised between half a metre and two metres. In some of the more important experiments the channels were con-

* Report on the "Physics and Hydraulics of the Mississippi River." By Captain A. A. Humphreys and Lieutenant H. L. Abbot. Philadelphia: 1861.

† Boileau: "Traité de la mesure des eaux courantes." Paris: 1854. Darcy: "Recherches experimentales relatives au mouvement de l'eau dans les tuyaux." Paris: 1857. Darcy et Bazin: "Recherches Hydrauliques." Paris: 1865. This last book constitutes a memoir by Bazin on researches commenced by Darcy, and continued for some time by him with the aid of Bazin; and, after the death of Darcy in 1858, continued by Bazin, and by him completed and worked out in the discussion of their results.

structed in wood, and were open above, and had a flat bottom and vertical sides, so that the current was rectangular in cross-section. Channels of various other forms were also used, and the mode of flow of the water in them was scrutinized. The results arrived at by these experimenters tend very much towards establishing the supposition which forms the subject of the present paper—the supposition namely of the prevalence or frequent occurrence of a distribution of velocities having the maximum velocity not at the surface but at some moderate depth below. Boileau, by his experiments, was led to announce as one of his conclusions (page 308), that in the medial longitudinal vertical section of a rectangular canal with uniform *régime*, the maximum of velocity is situated not at the surface, but at a depth which is a fraction more or less considerable of the total depth of the current. He also announced, as a conclusion, that the decrease of velocity, from the place of maximum velocity up to the surface, must be attributed to some new cause different from that which produces the diminution of velocity from the place of maximum down to the bottom. This new cause, he says, cannot be solely the resistance of the bed of air in contact with the liquid surface acting like the face of a pipe or conduit; and he assigns, in proof of this, the reason that the mobility of this bed of air does not permit of our attributing to it a retarding influence so great as that which is implied in the rapid abatement of velocities in approach towards the surface in the upper part of the current. He recounts his own special experiments, made in 1845, on the influence of wind on the velocities in currents,—a subject which he says had up to that time been very little investigated by hydraulicians. He deduces from his experiments conclusions (page 313) to the effect that in spite of varied disturbances produced by wind blowing over the water with varied intensity, yet there is manifested a very sensible tendency to a decrease of velocities of the water for approach towards the liquid surface; and that the maximum velocity is yet below the surface, even when the wind blows forward with the current, and has a velocity greater than that of the current. Judging, then, that resistance of the air cannot be the cause of the phenomenon, he says that it is then principally in the mutual actions which bind among one another the liquid particles, and in the oblique and rotatory movements which result, under the influence of these forces, from the difference of velocities of neighbouring particles, that it is necessary to seek for the explanation of the phenomena of the decrease of velocities in the approach towards the surface of currents. He goes on to say that we have to conceive, in fine, that these oblique movements, producing transverse living forces ("*forces vives*"), diminish according to certain general laws, the living forces of forward motion which the hydrometric instruments are adapted to indicate.

I have cited this passage from Boileau very fully, because it seems

to me to contain the nearest approach towards an explanation of the phenomenon in question of any that have been attempted, so far as any such attempts have come under my notice. It involves, I think, at least a glimmering towards a true explanation; but I regard it as being in great part erroneous, and importantly so in principle, and as being besides altogether incomplete. I do not think it has been offered by the very able investigator himself, who has proposed it, as being at all sufficient; but I think it has been offered only as tending to throw some light over the region for further search, and some indication towards courses in which speculation and research might well advance.

Bazin's experiments, of the general character already mentioned, were very extensive in their scope, and were carried out in great detail, and with some remarkable refinements of method. The velocities were measured mainly or wholly by a modification devised by Darcy of the well known instrument called Pitot's tube. Bazin, in the case of canals not very wide relatively to the depth of the current, found very clearly and decisively the phenomenon in question of the maximum velocity being below the surface. But, in the case of rectangular channels of more considerable width, channels having the width of the current so much as four or five times the depth or more, Bazin by his scrutiny and consideration of his experimental results, was led to conclude that the diminution of velocity for approach towards the surface in the upper part of the current is to be found only in the side parts of the current—the parts flowing along the two side walls. He judged that throughout the whole of the current, except two side parts, each having some moderate width, which might be equal to about twice the depth of the current, the maximum of the velocities for all points, situated in a vertical line, is to be found at the surface; and that the rate of diminution of velocity for descent from the surface would begin as nothing at the surface, and would go on increasing with descent to the bottom. His experiments, according to his own careful analysis and combination of them, appeared to be in agreement with this assumption, or to bring this supposition out as a result.

I do not, however, regard this conclusion as being trustworthy. His experiments for the case of great width relatively to depth had not, in any instance, a depth of water exceeding ·38 of a metre, or $1\frac{1}{4}$ foot, and thus the depths were so small absolutely as not to admit of a fine enough discrimination of minute changes of velocity for minute changes of depth of the point where the velocity was observed, nor of measuring velocities close enough to the surface. So far as experimental researches go, some doubt I presume must still remain over this part of the subject. Indeed, the Indian experiments, next to be mentioned, show results in disagreement with this conclusion offered by Darcy.

Quite recently, in 1874–75, experiments were conducted in India on the Ganges Canal, close to Roorkee, by Captain Allan Cunningham, R.E.* These experiments bring out among their results, very remarkably, the frequently alleged phenomenon of the maximum velocity of the water being not at the surface, but at some moderate depth below. And further, it is deserving of special notice that those of his experiments, which have chiefly to be referred to as throwing light on this subject, were made in an aqueduct about 85 feet wide, and with an approximately level bottom; and that the depths of the water in different experiments ranged from about 6 feet to about $9\frac{1}{2}$ feet, so that the width was on different occasions from about nine times to about fourteen times the depth, and yet the maximum of the velocities at mid-channel (or the maximum velocity in the longitudinal medial vertical section) came out by averages of numerous results, and, by varied modes of experimenting, to be very decidedly below the surface.

Experiments carried out lately on a very large scale on the Irawaddy river by Robert Gordon, Executive Engineer, British Burmah, Public Works Department, go to confirm the truth of the same phenomenon. These experiments of Mr. Gordon, however, although valuable in many respects, appear to be subject to some doubt as to whether, through the mode of experimenting, the level of supposed maximum velocity has not been brought out too low, that is to say, too near the bottom. On this point Mr. Gordon (in his Introductory Note, § vii, page ii of date 16th June, 1875) intimates his intention to make further experiments with other instruments, but still asserts his confidence in his previous methods and results.

Until about two years ago I had not happened to become acquainted with any of the evidence for the phenomenon in question except the unsatisfying experimental results given by Ellet; but about two years ago I met with accounts of some of the more recent and more convincing experimental investigations. It then appeared to me that if the asserted phenomenon must really be accepted as a truth, there ought to be some mode possible of accounting for it: and a theory occurred to me which I now propose to submit.

The mode of thought which near the beginning of the present paper I have described as constituting the laminar theory, I must premise, has long appeared to me to be an erroneous and a very misleading view. It was a very prevalent mode of thought, and was usually too influential on people's minds even when they did entertain decidedly, though often not clearly enough, the consideration of eddies and transverse movements or commingling currents with different velocities.

* "Hydraulic Experiments at Roorkee, 1874–75," by Captain Allan Cunningham, R.E., published in "Professional Papers on Indian Engineering." Thomason, College Press, Roorkee, 1875: also Spon and Co., London, &c.

The great distinction between the mode of flow of a very viscid fluid, such as treacle or tar, and the mode of flow of water in ordinary circumstances in pipes and in open channels, has not been enough generally and enough consistently attended to. The laminar theory constitutes a very good representation of the viscid mode of motion; but it offers a very fallacious view of the motion in the flow of water in ordinary cases in which the inertia of the various parts of the fluid is not subordinated to the restraints of viscosity.

In the flow of water in an open channel in ordinary circumstances the earth's attraction is perpetually tending to accelerate the forward motion of the water throughout the whole body of the current in consequence of the surface declivity; or we may say, with more complete expression, in consequence of the fall of *free-level** which, in virtue of the surface declivity, occurs to all particles in the current as they advance in their down-stream course. The tendency to increase of velocity, if we neglect the backward or forward force, usually very small, or it may be nothing, applied by the air to the water surface, we may say is counteracted solely by a backward resisting force-system applied by the wetted face of the channel to the water momentarily in contact with it. The wetted channel face, it must be observed, is ordinarily more or less rough with gravel, mud, weeds, or other asperities. It is not a true view to imagine a smooth channel face washed by a thin lamina of water, which imagined lamina of water receives a backward or resisting force-system applied tangentially by the so imagined channel face, and transmits tangential backward force to another lamina of water lying next to itself on the side remote from the channel face. It is not the case that from any layer of water whatever, thick or thin, spread over the channel face, resisting forces are transmitted to the interior of the body of the current in any great degree by mere viscid resistance to change of form in the intervening fluid, as would be the case if it were like treacle or tar. But, very differently, indefinite increase of velocity of the water situated in the interior of the current is prevented by continual transverse flows thereto, and commingling therewith, of portions of water already retarded through their having been lately in close proximity to the resisting channel face; and, jointly with that, by the condition that portions of the fluid which have been flowing forward temporarily in

* The *free-level* for any particle of water, in a mass of statical or of flowing water, is the level of the atmospheric end of a column, or of any bar of statical water, straight or curved, having one end situated at the level of the particle, and having at that end the same pressure as the particle has, and having the other end consisting of a level surface of water freely exposed to the atmosphere, or else having otherwise atmospheric pressure there. Or, briefly, we may say that the *free-level* for any particle of water is the level of the atmospheric end of its pressure-column, or of an equivalent ideal pressure-column.

the interior of the current, and have been gaining forward acceleration there are gradually expelled, or do gradually flow from that region, and come themselves into close proximity to the resisting channel face; and so, in their turn, do receive very directly backward forces from the face, because in proximity to it processes of fluid distortion subject to viscid resistance are going on with great activity and intensity.

The transverse motions have their origin primarily in the rush of the water along the wetted channel face. When that face is rough or irregular with lumps and hollows or other asperities, reasons for the origination of transverse currents may be sufficiently obvious. But even if the channel face is extremely smooth, so as to present no sensible asperities, still there is good reason to assert that transverse flows will come to be instituted in consequence of the rapid flow of the main body of the current along a lamina, very thin it may be, of water greatly deadened as to forward motion by viscid cohesion with the channel face, and throughout and across which, if regarded as only very thin, in virtue of its thinness, the backward force applied by the face can be transmitted by mere viscosity. The thin lamina of deadened water will tend by the scour of the quicker going water always moving subject to variations both of velocity and of direction of motion to be driven into irregularly distributed masses; and these, acted on by the quicker moving water scouring past them, will force that water sidewise, and will be entangled with it and will pass away with some transverse motion to commingle with other parts of the current.*

If we watch the surfaces of flowing rivers, or of tidal currents flowing in narrows or *kyles*, we may often have opportunity to observe very prevalent indications of rushes of water coming up to the surface and spreading out there. These rushes often may be seen to keep rising in quick succession in numerous neighbouring parts of the

* This principle I noticed myself in the connexion in which it is here adduced; and the idea has since been confirmed to me and rendered more definite through additional considerations mentioned to me lately by my brother, Sir William Thomson, which have originated with him in some of his theoretical investigations in quite another branch of hydraulic science, and which relate to finite slip in a frictionless fluid. He pointed out that if, for water theoretically regarded as frictionless, or devoid of viscosity, we imagine a long smoothly formed straight trough or channel with a thin vertical longitudinal plane septum dividing it into two parts each uniform in cross-section throughout its length, and if we imagine the space on one side of the septum to be occupied by still water, and a current to be flowing along on the other side; and if, while this is in progress, we imagine the vertical partition to be withdrawn so as to leave the current flowing along a plane face of still water, the motion with the finite slip thus instituted will be essentially unstable. Reasons for this, when once it is brought under notice, are very obvious from consideration of the centrifugal forces, or centrifugal actions, which would be introduced on the slightest beginning being made of any protuberance or hollow in the originally plane interface between the still water and the current.

water surface, and they may be seen presenting appearances of spreading out till they meet one another and give indication of momentary downward sinking at their places of meeting.

From whence do these transverse currents come to the surface? It seems to me they must have had their origin in the deadened water scouring along the bottom, or along the wetted side-faces of the channel, in such ways as have just now been briefly sketched out. Thus it seems that there are tendencies bringing about the result that the superficial stratum of the river receives perpetually renewals of its substance by water currents arriving to it, and spreading out there, which have very recently departed from the bottom before coming up to enter into that superficial stratum. But their substance, having come in great part from the bottom, must be largely made up of the deadened or slow-going bottom-water. It is to be understood that this deadened water, in rising through the current towards the surface, is partly urged forward in the down-stream direction by the surrounding quicker-going water, but that it arrives at the surface without having attained fully to the down-stream velocity of that intermediate stream.

It may readily be perceived that it is from the washed face of the channel alone, or from that and the retarded layer of water in proximity to it, that any strong transverse impulses can be applied to any parts of the current. No rapid transverse current will originate in the middle of the body of the river; for there is no cause for the origination of transverse currents there, unless perhaps we were to regard as such any slight transverse motions which may be produced through the gliding forward of parts of the water there relatively to others near them going with different velocities, and unless we were to regard as such any transverse disturbances that may be imparted to forward-flowing water there by the intrusion and commingling of partially deadened water from the channel-face.

We may now have great confidence, I think, in taking as a well-established truth, or at least as a very probable view, the supposition already laid down to the effect that very commonly the superficial stratum of a river receives perpetually renewals of its substance by water currents arriving to it and spreading out there, which have very recently departed from the bottom or sides of the channel before coming up to enter into that superficial stratum; and that the substance thus perpetually renewing the surface stratum is largely composed of deadened or slow-going bottom-water, or of water going slower forward than the water through which it traverses in ascending to the surface. It is further to be noticed that the water which at any moment constitutes the superficial stratum is, in its turn, very soon overflowed by later arrivals from the bottom. So it gradually descends from the surface into the interior of the body of the river. But during this action it is always flowing downhill, or we may better say it is experiencing

a fall of free level, in consequence of the surface declivity. It is thus receiving forward acceleration in the downhill direction, and its velocity goes on increasing until at some depth from the surface it reaches a maximum, from whence, during further lapse of time and further descent of this water towards the bottom, the retarding influences imparted to it from the bottom are predominant over the downhill accelerating influence of gravity. These retarding influences, chiefly acting through transverse rushes of water from the bottom commingling more numerously and more briskly with the descending water under consideration the more it gets into the neighbourhood of the bottom, bring about the result that the water goes forward with less and less velocity as it approaches nearer and nearer to the bottom.

I have now to offer, by consideration of an imaginable case different from that of an ordinary river, an illustration which will aid in the forming of clear ideas on what I have been presenting as a true theory of the real behaviour of the water in rivers.

Let us imagine a flowing river composed mostly of water, but with a layer of oil floating on the top, the oil being of some such depth as a tenth or a twentieth part of the whole depth of the river. Let us suppose the width of the river to be so very great relatively to the depth as that in considering the flow in a middle portion of the river, we may regard it as experiencing no sensible retarding influences, either through the water or the oil, from the sides of the river; and let the flow to be kept under consideration be only that middle portion without the lateral portions which would be sensibly affected by retarding influences from the sides. Here we have a case differing from that of an ordinary river of water in this important respect, that, while in the ordinary river the superficial stratum of fluid is perpetually changing its substance, and is, as I suppose, perpetually receiving new supplies of deadened water from the bottom, in the imagined case now adduced the substance of the superficial layer being of oil floating at top, does not undergo any such change. The oil then, it seems very certain, would really rush down what we may call the inclined plane of water on which it lies, and would go on accelerating its motion until, by advancing very much faster than the water, it would introduce a frictional drag between itself and the water sufficient to hinder its further acceleration;* or rather until, without attaining to

* *Postscript note*, 1*st November*, 1878.—An observed phenomenon, which, if duly taken into consideration, must doubtless be found to be closely allied in its nature to the supposed behaviour of the imagined layer of oil on a flowing river of water above adduced, and which is certainly of much interest, both for its own sake and in reference to theoretical views which have been held as to its origin and its indications, has come under my notice since the time when the present paper in manuscript was presented to the Royal Society. The book by Bazin, which may be briefly named as Darcy et Bazin "Recherches Hydrauliques," Paris, 1865 (*see* a previous foot-note in this paper), contains prefixed to it a report, dated 1863, of a committee of the

that stage of great relative velocity, it would at an earlier stage ruffle up the mutual face of meeting of itself and the water into protuberances and hollows, somewhat like waves, on the principle referred to already in a foot-note as having been proposed by Sir William Thomson, and would carry this action on to the extent of causing commotion and commingling of the water and oil. The contrast between this case and that of an ordinary river of water is so remarkable as to aid the forming of a clear comprehension of the very different mode of action which I have been attributing to the water in ordinary rivers and other open channels.

It is further worthy of notice that if, from any local cause, the water flowing forward in some part of the width of a river has in its motion a component downward from the surface towards the bottom, and is free from intrusion of upward currents or rushes of deadened water

Academy of Sciences on the memoir of M. Bazin, "Sur le Mouvement de l'Eau dans les Canaux decouverts." In that report the committee remark (as confirmatory of the view which they accept, to the effect that in deep rivers, especially when not very wide relatively to their depth, the place of maximum velocity is at a considerable depth below the surface) as follows :—"Il y a longtemps que les bateliers du Rhin et nos pontonniers savent qu'un bateau chargé et ayant un fort tirant d'eau, marche, en descendant, plus vite que l'eau qui le soutient ou que les corps flottants à la surface." This obviously conveys the opinion that a heavily loaded boat, sinking deep into the water, and thereby having its deeper part immersed in water which is flowing quicker than the surface water, is dragged forwards by that deeper and quicker moving water, and so is made to advance quicker than the surface water does. The idea seems to be that the boat has some average velocity less than that of the water at its bottom, and greater than that of the surface water. The view which thus appears to be held in respect to the observed phenomenon seems to me to be inadequate and erroneous. On the principle put forward above in the present paper in reference to the imagined case of a river with an upper layer of oil, I would suppose that a large and heavy boat, even if flat-bottomed and of shallow draught of water, would run down the river-course quicker than the water in which it swims; for the reason that while all the water surrounding it makes occasional visits to the bottom of the river, and meets with great retardation there, the boat does not dive to the bottom, and is free from any such retardation, and so is only held back by the surrounding water against taking from gravity a perpetually increasing velocity. Thus it must go faster than the surrounding water which has to hold it back. The boat of deeper draught referred to by the committee I would suppose would advance quicker than the surface water, for the same reason, and not merely because of its bottom being situated in water moving quicker than that at the surface. The principles I have assigned would afford ample reason for our supposing that the boat of deep draught might swim forward much quicker not only than the surface water, but also than the water at its bottom, or indeed than any part of the water of the river surrounding the boat. Very small floating objects, such as sticks or leaves, would present, in proportion to their small masses, so much resistance to motion through the surrounding water that they would be constrained in fact to move sensibly at the same velocity as that of the water surrounding them. The phenomenon would thus be presented of the boat swimming forward past the small floating objects around it.

J. T.

from the bottom, or of water retarded by the influence of the river-bed, we ought to expect the forward velocity to increase from the surface to very nearly the bottom. The accelerative influence of gravity due to the surface inclination, and more particularly due to the fall of free-level experienced, as an accompaniment of that inclination, by the water throughout the body of the current in its onward flow would generate in every portion or particle of this water increase of velocity for advance along its course; because, in the absence of rushes of deadened water from the bed, such as it appears do commonly intrude into the body of the current, there would be no retardative influence to counteract the gravitational accelerative influence; since the mere viscosity of the water unaided by transverse commingling is, I consider, insignificantly small and quite ineffectual as a resisting influence or means of transmitting resistance from the bed to any part of the water in the body of the current out of close proximity to the bed. But as this forward moving water is also descending towards the bottom while it is gaining forward velocity, it follows that, in the circumstances of flow supposed, we ought to expect the forward velocity to increase with descent from the surface to very nearly the bottom. It is to be understood that the freedom supposed from upward rushes or intrusions of deadened water will not be maintained in the water when it arrives into proximity to the bottom. In approaching very near to the bottom the water must begin to receive important resisting forces communicated to it from the bottom through commingling of deadened water, and by intense distortional actions with viscosity.

It is also to be noticed in connexion with the case under consideration that if, in one part of the width of the river, there is a prevailing descent towards the bottom, there will be upward flows to compensate for this in other parts of the width. Then obviously the whole character of the action of the water will be very different in the regions where ascent prevails from that in the regions where there is a prevailing descent; and the distribution of forward velocities throughout any vertical line in the one region will be quite different from the distribution of forward velocities throughout any vertical line in the other region. Local circumstances casually affecting the flow in the way here described I think may perhaps account for some of the apparent anomalies in respect to the distribution of velocities through different parts of the depth from surface to bottom which have been met with by various experimenters, and have been included among the recognised causes of the perplexity and bewilderment with which this branch of hydraulic science is pervaded.

I wish next to draw attention to one of the results of observation and experiment announced by Captain Cunningham in his book already referred to ("Hydraulic Experiments at Roorkee"). In his discussion of his experimental results on the flow of water in each of

two artificially-formed channels on the Ganges Canal, one of them, 168 feet wide, and the other 85 feet wide, and each having the water often about from 6 feet to 9 feet deep, he states (p. 46, article 35): "There is a constant surface motion (deviation) from the edges towards the centre, most intense at the edges and rapidly decreasing with distance from the edges."

This experimental conclusion, on the supposition of its being decidedly trustworthy, as Mr. Cunningham asserts with confidence that it is, I think may probably be satisfactorily explicable through considerations intimately connected with those which I have already given for an amended theory of the flow of water in rivers.

I wish, however, not to prolong the present paper by entering on any detailed discussion of this branch of the subject, and besides I prefer to reserve this for some further consideration before venturing to put forward the views in reference to it which at present appear to me likely to be tenable. It may be noticed, however, that Captain Cunningham's experimental result, if decidedly correct, throws additional light on the subject of the abatement of surface velocity comparatively to the velocity at some depth below the surface being found in Bazin's experiments to occur in a much greater degree near the sides of rectangular and various other channels than at middle. Bazin thought indeed from his own experiments (as I have already had occasion to mention) that the relative retardation or slowness of the surface occurred not in the middle of wide channels (that is to say, of channels wide relatively to the depth of the water) but only near the sides; but this supposition I have referred to as appearing not to be trustworthy. With these brief suggestions I will now leave for further consideration the subject of the special phenomena of the influence of the sides.

Historical Note.

Subsequently to my having formed, in all its primary or more essential features, the new view now explained of the flow of water in rivers, and before I had met with the book of Humphreys and Abbot, I happened to see in the writings of another author (paper of Mr. Gordon already referred to) the following remark in reference to their views as to the velocity at the surface being less than at some depth below. "Humphreys and Abbot attribute the fact to transmitted motion from the irregularities of the bottom; but confess themselves dissatisfied with their own explanation."

These words seemed to me to indicate a probability of Humphreys and Abbot having anticipated me in some part at least of the theory which I had been forming. On obtaining their book, however, and reading the passage referred to, not by itself alone, but with its context, it appeared to me that it involved no real anticipation,

although one clause of a sentence in it, read by itself, might be supposed to do so. The passage is to be found in their work at p. 286. They begin by saying, that their experimental observations detailed in their previous pages "prove that even in a perfectly calm day there is a strong resistance to the motion of the water at the surface as well as at the bottom," and that this resistance at the surface "is not wholly or even mainly caused by friction against the air." They go on to say:—"One important cause of this resistance is believed to be the loss of living force, arising from upward currents or transmitted motion occasioned by irregularities at the bottom. This loss is greater at the surface than near it. The experiment of transmitted motion through a series of ivory balls illustrates this effect. It is likewise illustrated on a large scale by the collision of two trains of cars on a railway, in which case it has been observed that the cars at the head of the train are the most injured and thrown the farthest from the track; those at the end of the train are next in order of injury and disturbance; while those in the middle of the train are but little injured or disturbed. Other causes may and probably do exist, but their investigation has, fortunately, more of scientific interest than practical value. For all general purposes it may be assumed that there is a resistance at the surface, of the same order or nature as that which exists at the bottom."

Now although this passage does contain the words "*arising from upward currents or transmitted motion occasioned by irregularities at the bottom*," yet the illustrations, by means of the series of ivory balls, and of the collision of railway trains, show that the authors attribute to those words no clear and correct meaning, but, on the contrary, I would say they put forward quite a false view of the actions going on. Besides I myself do not admit that, except from the air, there is a resistance at the surface. According to my supposition the already resisted and retarded bottom water comes to the surface and spreads out there, but receives no new resistance there, and on the contrary receives acceleration from gravity in running down hill.

II. "The Magic Mirror of Japan." Part I. By Professors W. E. Ayrton and John Perry, of the Imperial College of Engineering, Japan. Communicated by William Spottiswoode, Esq., M.A., Treas. R.S., &c., &c. Received October 2, 1878.

The Japanese mirror must, from three points of view, attract the notice of foreigners sojourning in that country—its prominence in the temples, the important feature it forms in the limited furniture of a Japanese household, and the wonderful property (which has apparently